explaining their structural relationships to human races as inheritances from a common basal stock.

It will be thus seen that Prof. Sergi is the arch-priest of that heterodox doctrine—the multiple origin of closely allied species and genera. His faith is more robust than that of the majority of his colleagues. He accepts implicitly Ameghino's speculations concerning the independent origin of mankind in South America. Although the reviewer regards the majority of Prof. Sergi's opinions as ill-founded, he is only too willing to admit that it would be presumptuous, in the present state of our knowledge of extinct forms, to refuse them a most careful investigation.

A. K.

Vicious Circles in Disease. By J. B. Hurry. Second edition. Pp. xiv+280. (London: J. and A. Churchill, 1913.) Price 7*s.* 6*d.* net.

IN the issue of NATURE for May 18, 1911 (vol. lxxxvi., p. 374), an extended review by Sir T. Clifford Allbutt was published of the first edition of Dr. Hurry's book. The present edition has been revised, and six new chapters have been added in the hope of covering the ground more adequately. Most of the material of these additions has appeared already in the medical Press.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Soil Fertility.

DR. RUSSELL begs the whole question in two lines of his letter in NATURE of January 16, when he terminates para. 7 with "the increased gain in plant growth on such highly heated soils can be largely attributed to this cause," viz. to the formation of ammonium and other simple soluble nitrogen compounds on heating soils to 170°.

If this were true, then the effects of heating soils, whether to the temperature of "partial sterilisation," viz. 98° (as in Dr. Russell's experiments) or (as in mine) to 170°, could be imitated by adding in, say, daily doses, suitable solutions containing ammonium compounds and nitrates.

I have repeatedly tried this with various combinations of salts, both in pot experiments and in the field, and have invariably found that the increased growth due to heating the soil previously was never even approached in extent by that in any of the plots or pots to which the manures were added.

It appears to me that the increased growth in Dr. Russell's experiments can only be safely ascribed to the manufacture of soluble nitrogen compounds by bacteria when in parallel sets of pots and plots the same effect is shown to be produced by artificially dosing unheated soils with such nitrogen compounds.

F. FLETCHER.

Rewika Ranch, Kyambu, British East Africa, March 6.

I AM not at all surprised that Mr. Fletcher failed to reproduce the conditions of a strongly heated soil by simply adding frequent doses of ammonium compounds to an unheated soil. Soil suffers considerable decomposition when heated to 170° C., and changes markedly in chemical composition, physical properties, suitability as a medium for the growth of bacteria, moulds, and other organisms, and as a habitat for the higher plants. Experiments become extraordinarily difficult to interpret when so many factors change simultaneously, and for this reason I have always preferred to adopt very much milder methods, treating the soil with antiseptic vapours (*e.g.* toluene), or heating to as low a temperature as possible (60° to 95° C.). Here less complication arises, because the decomposition effects are at a minimum, and one can study the various factors one at a time.

Increases in productiveness equal to those brought about by treatment with antiseptic vapours or heating to 65° C. can be obtained on our normal untreated soils by additions of sodium nitrate or ammonium sulphate. Further, partial sterilisation has failed to bring about increased productiveness when the treated and untreated soils are subsequently so liberally treated with nitrogenous plant food that the nitrogen supply is no longer a limiting factor. In "sick" soils, however, there is another limiting factor, the presence of disease organisms and pests, and this also is put out of action more or less completely by partial sterilisation. Here addition of nitrogenous plant food (which leaves the pests unaffected) does not make the untreated soil equal in productiveness to the partially sterilised soils. We could get no evidence of the toxin suggested by Mr. Fletcher, and, this being the case, I do not see how we shall advance matters by assuming its presence as a third limiting factor.

E. J. RUSSELL.

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Induced Cell-reproduction in the Protozoa.

I WAS interested in Mr. T. Goodey's letter under the above heading in NATURE of March 13, but should like to make a few remarks thereon. Hay infusion, which Mr. Goodey states caused the excystation of *Colpoda cucullus*, is prepared from dried grass, and here we have the products of cytolysis, and, in consequence, should expect the presence of auxetics. As a matter of fact, any vegetable infusion contains auxetics, the presence of which can be demonstrated by the jelly method on human lymphocytes, as described by Dr. H. C. Ross, "Induced Cell Reproduction and Cancer" (London: John Murray, 1910). Encysted forms of Colpoda cannot be compared with the winter spores of Polytoma, as in Colpoda there is, so far as I can gather from Mr. Goodey's letter, no conjugation prior to the encystment, and consequently no real development is necessary for the excystation, but only rupture of the cyst-wall. In Polytoma, however, the cytoplasm within the spores has to undergo several complex changes, leading ultimately to division of sarcode, formation of envelopes round the products of division, and the development of flagella. Thus, I take it that in Colpoda there is no reproductive process in the excystation, and consequently no necessity for auxetics; anything that will cause the rupture of the cyst-wall being sufficient, although, as already shown, auxetics were certainly present in the hay infusion.

With regard to the "pure distilled water," Mr. Goodey should remember that this is pure only so long as he adds nothing else to it. Directly organisms are added, auxetics would be present, as, apart from the fact that some of the culture fluid would be introduced with the organisms, even if this were not so, auxetics would be present, as there would be necessarily some death-rate. The same phenomenon also occurs in pond Amœbæ, the encysted forms of which can also be caused to undergo excystation by incubation with distilled water.

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Whether auxetics are necessary for any form of cell-reproduction to occur is a point which will require further research to determine. It is, however, a striking fact that Dr. H. C. Ross was able for the first time to induce divisions in human leucocytes by means of auxetics, and was also able to demonstrate that the ova of *Ascaris megalocephala* will undergo division if incubated with auxetics. Dr. Fansham has also shown that *Entamoeba coli* can be caused to divide through many generations by means of these substances, whilst Dr. E. H. Ross has demonstrated that auxetics have a very remarkable action on trypanosomes.

From the foregoing facts it is clear that auxetics are *a cause* of cell-reproduction, and, although we cannot as yet state positively that there are no other causes, yet, judging from other biological examples, it is extremely probable that they are the sole cause, as it is very unlikely that a complex function like cell-reproduction should have more than one direct cause. With regard to the presence of auxetics and kinetics in pond water, I may say that I am at present investigating this point, and have definite proof of the presence, not only of auxetics, but also of kinetics or augmentors, in such waters, the latter bodies apparently varying according to the season, and also being dependent on the amount of albuminoid ammonia present.

Besides the presence of auxetics in hay infusion, there is one further point to be mentioned, viz. that if by the action of an enzyme, the cyst-wall in Colpoda were dissolved, quite enough auxetic would probably be liberated to cause division, were such necessary for development. This was well shown by Dr. H. C. Ross, who found that substances not themselves auxetics may yet have auxetic action by causing limited cell death immediately within the walls of ova, thus setting free enough auxetic to cause cell-division.

AUBREY H. DREW.

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Units of Pressure in Vacuum Work.

REFERRING to the letter by Mr. Shaw in NATURE of March 20 (p. 95), I beg leave to remind readers that we have already a convenient unit of pressure which, as fitting in an absolute system of units, is preferable to the micron of mercury, viz. the dyne per cm.^{2}, or the barye of the c.g.s.-system. In fact, Prof. Knudsen has used it in all his later researches on molecular phenomena. In article V10 of the "Encyklopaedie der mathematischen Wissenschaften," p. 628, note 19 (Communications from the Physical Laboratory at Leyden, Suppl. No. 23, p. 14), by Prof. Kamerlingh Onnes and myself, we have given practically the same unit under the name of *millitor* as convenient for such pressures as those in Röntgen vacua.

In doing this we have followed the lead of the commission of the International Association of Refrigeration (*Bull. de l'Ass. internat. du froid*, 2, 1911, p. 38, rapporteur M. Ch. Ed. Guillaume). This commission proposed to accept the metre-kilogramme-second system for general use, this one having better chances than the c.g.s.-system, and accordingly to introduce as an absolute unit of pressure the m.k.s.-unit. As a practically identical realisation of it the commission proposed to introduce the *international centitor* (abbreviated for centi-torricelli), the international kilotor being equal to the pressure of a column of practically 75 cm. of mercury under normal gravity (for further particulars see the article quoted above). Practically 1 millitor = 1 dyne/cm.^{2}, or barye, and within the accuracy of experiments in the domain of these vacua 1 millitor = 0·75 μ of mercury.

Seeing how simple this proportionality factor is, the work of reducing, say, McLeod gauge readings to millitors will not cause any appreciable trouble, whereas the indications by Prof. Knudsen in dynes/cm.^{2} are without any reduction expressed in millitors. For the highest vacua the *microtor* = 10^{-3} millitor = $0{\cdot}75 \times 10^{-6}$ mm. of mercury would be convenient. As abbreviations, mtor may be written for millitor, μtor for microtor.

W. H. KEESOM.

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Reflection of X-Rays and X-Ray Fringes.

ACCOUNTS of the reflection of X-rays and of X-ray fringes contributed to NATURE by Messrs. Bragg, Moseley, Barkla, Hupke, Keene, and others induce me to send you some results that I have obtained recently in the same direction.

I think that the appearances observed by Messrs. Laue, Friedrich, and Knipping are really due to the same cause as the reflected spots; they present quite a similar distribution and general character.

Fig. 1 shows the pattern obtained with a beam of Röntgen rays falling at an incidence of 80° upon a face of a cube of rock-salt, the photographic plate being at right angles to the reflected beam. The reflected spots are similar to the transmitted spots, and present fringes perpendicular to the plane of incidence; they are situated on an elliptic curve, to which belongs also the point of impact of the primary beam. The spot on the main axis is regularly reflected; the others are symmetrically disposed, and possibly due to reflection on the planes of the corresponding octahedron, dodecahedron, &c., which are in suitable positions. Fluorine gives the same pattern.

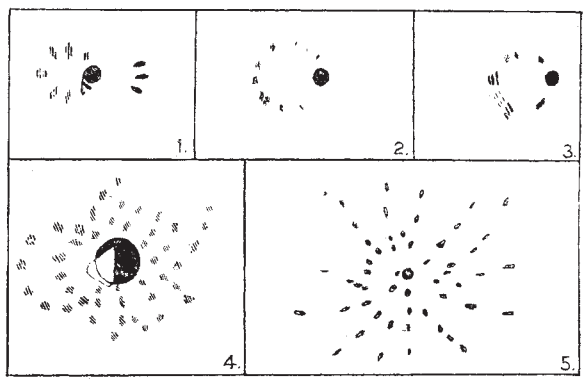

Fig. 2 is obtained with a beam (incidence 80°) falling upon the triangular face of an octahedric crystal of magnetite. The reflected spots show two systems of fringes, one of which is approximately perpendicular to the plane of incidence.

It seems of importance to pay attention to the angle between the intersection of the plane of incidence and the quaternary axis situated in the cubic face of reflection. Fig. 3 shows that when this angle (which was 0 in Fig. 1) becomes 45°, the curve of spots is deflected, the regularly reflected spot being no longer on the main axis but following the ordinary law of reflection; the distance of the fringes is probably slightly changed in this case.

I have often observed fringes in the transmitted spots obtained by Laue's methods. With octahedric magnetite (Fig. 4) all the spots (more than 100) were striated by parallel fringes; and on a plate (Fig. 5) obtained with fluorine the transmitted spots, the number of which was also very large, are all doubled in a radial direction.

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